XXII. *New Experiments in Electricity: In a Letter from Mr.* Ebenezer Kinnerſley, *to* Benjamin Franklin, *LL. D. F. R. S.*

Read Nov. 18, 1762, March 24, and April 14, 1763.

SIR, Philadelphia, Mar. 12, 1761.

HAVING lately made the following experiments, I very chearfully communicate them, in hopes of giving you ſome degree of pleaſure, and exciting you to further explore your favourite, but not quite exhauſted, ſubject, ELECTRICITY.

EXP. I.

I placed myſelf on an electric ſtand, and, being well electriſed, threw my hat to an unelectriſed perſon, at a conſiderable diſtance, on another ſtand, and found, that the hat carried ſome of the electricity with it; for, upon going immediately to the perſon, who received it, and holding a flaxen thread near him, I perceived he was electriſed ſufficiently to attract the thread.

EXP. II.

I then ſuſpended, by ſilk, a broad plate of metal, and electriſed ſome boiling water under it, at about four feet diſtance, expecting that the vapour, which aſcended plentifully to the plate, would, upon the principle of the foregoing experiment, carry up ſome of the electricity with it; but was at length fully convinced, by ſeveral repeated trials, that it left all its ſhare

ſhare thereof behind. This I know not how to account for; but does it not ſeem to corroborate your hypotheſis, that the vapors, of which the clouds are formed, leave their ſhare of electricity behind in the common ſtock, and aſcend in a negative ſtate?

EXP. III.

I put boiling water into a coated Florence flaſk, and found that the heat ſo enlarged the pores of the glaſs, that it could not be charged. The electricity paſſed thro' as readily, to all appearance, as thro' metal; the charge of a three-pint bottle went freely thro' without injuring the flaſk in the leaſt. When it became almoſt cold, I could charge it as uſual. Would not this experiment convince the Abbé Nollet of his egregious miſtake? For, while the electricity went fairly thro' the glaſs, as he contends it always does, the glaſs could not be charged at all.

EXP. IV.

I took a ſlender piece of cedar, about eighteen inches long, fixed a braſs cap in the middle, thruſt a pin, horizontally and at right angles, thro' each end, (the points in contrary directions) and hung it, nicely balanced like the needle of a compaſs, on a pin, about ſix inches long, fixed in the center of an electric ſtand. Then electriſing the ſtand, I had the pleaſure of ſeeing what I expected; the wooden needle turned round, carrying the pins with their heads foremoſt. I then electriſed the ſtand negatively, expecting the needle to turn the contrary way; but was extremely diſappointed, for it went ſtill the ſame way as before.

When the ſtand was electriſed poſitively, I ſuppoſe, that the natural quantity of electricity in the air being increaſed on one ſide, by what iſſued from the points, the needle was attracted by the leſſer quantity on the other ſide. When electriſed negatively, I ſuppoſe, that the natural quantity of electricity in the air was diminiſhed near the points; in conſequence whereof, the equilibrium being deſtroyed, the needle was attracted by the greater quantity on the oppoſite ſide.

The doctrine of repulſion in electriſed bodies, I begin to be ſomewhat doubtful of. I think all the phænomena, on which it is founded, may be well enough accounted for without it. Will not cork balls, electriſed negatively, ſeparate as far as when electriſed poſitively? And may not their ſeparation, in both caſes, be accounted for upon the ſame principle; namely, the mutual attraction of the natural quantity in the air, and that which is denſer, or rarer in the cork ball? It being one of the eſtabliſhed laws of this fluid, that quantities of different denſities ſhall mutually attract each other, in order to reſtore the equilibrium.

I can ſee no reaſon to conclude, that the air has not its ſhare of the common ſtock of electricity as well as glaſs, and, perhaps, all other electrics per ſe. For tho' the air will admit bodies to be electriſed in it either poſitively or negatively, and will not readily carry off the redundancy in the one caſe, or ſupply the deficiency in the other;

EXP.

EXP. V.

Yet let a perſon in the negative ſtate, out of doors in the dark, when the air is dry, hold, with his arm extended, a long ſharp needle, pointing upwards; and he will ſoon be convinced, that electricity may be drawn out of the air; not very plentifully, for, being a bad conductor, it ſeems loth to part with it; but yet ſome will evidently be collected. The air near the perſon's body, having leſs than its natural quantity, will have none to ſpare; but, his arm being extended as above, ſome will be collected from the remoter air, and will appear luminous as it converges to the point of the needle.

Let a perſon electriſed negatively preſent the point of a needle, horizontally, to a cork ball ſuſpended by ſilk, and the ball will be attracted towards the point, till it has parted with ſo much of its natural quantity of electricity as to be in the negative ſtate, in the ſame degree with the perſon who holds the needle: then it will recede from the point; being, as I ſuppoſe, attracted the contrary way by the electricity of greater denſity in the air behind it. But, as this opinion ſeems to deviate from electrical orthodoxy, I ſhould be glad to ſee theſe phænomena better accounted for by your ſuperior and more penetrating genius.

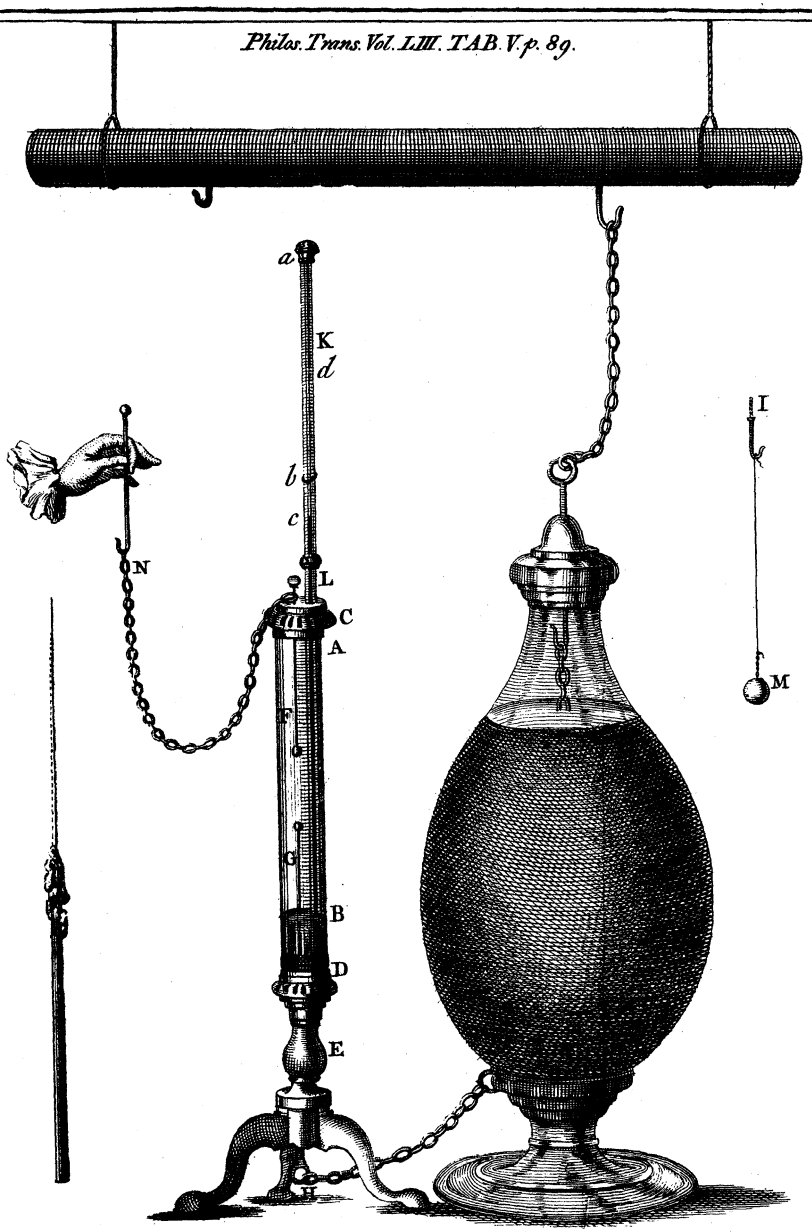
Whether the electricity in the air, in clear dry weather, be of the ſame denſity at the height of two or three hundred yards, as near the ſurface of the earth, may be ſatisfactorily determined by your old experiment of the kite.

EXP.

EXP. VI.

The twine ſhould have, through-out, a very ſmall wire in it; and the ends of the wire, where the ſeveral lengths are united, ought to be tied down with a waxed thread to prevent their acting in the manner of points. I have tried the experiment twice, when the air was as dry as we ever have it, and ſo clear that not a cloud could be ſeen; and found the twine each time, in a ſmall degree electriſed poſitively. The kite had three metalline points fixed to it; one on the top, and one on each ſide. That the twine was electriſed, appeared by the ſeparating of two ſmall cork balls ſuſpended on the twine by fine flaxen threads, juſt above where the ſilk was tied to it, and ſheltered from the wind. That the twine was electriſed poſitively, was proved by applying to it the wire of a charged bottle; which cauſed the balls to ſeparate further, without firſt coming nearer together. This experiment ſhewed, that the electricity in the air, at thoſe times, was denſer above than below. But that cannot be always the caſe; for you know we have frequently found the thunder clouds in the negative ſtate, attracting electricity from the earth. Which ſtate it is probable they are always in when firſt formed, and till they have received a ſufficient ſupply. How they come afterwards, towards the latter end of the guſt, to be in the poſitive ſtate, which is ſometimes the caſe, is a ſubject for further enquiry.

After the above experiments with the wooden needle, I formed a croſs of two pieces of wood, of equal



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equal length, interſecting each other at right angles in the middle; hung it, horizontally, on a central pin, and ſet a light horſe, with his rider, upon each extremity; whereupon, the whole being nicely ballanced, and each courſer urged on by an electriſed point, inſtead of a pair of ſpurs, I was entertained with an electrical horſe-race.

I have contrived an electrical air thermometer, and made ſeveral experiments with it, that have afforded me much ſatisfaction and pleaſure. It is extremely ſenſible of any alteration in the ſtate of the included air, and fully determines that controverted point, whether there be any heat in the electric fire. By the Plate [TAB. IV.] and the following deſcription, you will readily apprehend the conſtruction of it.

A. B. is a glaſs tube about eleven inches long, and one inch diameter in the bore. It has a braſs feril cemented on each end, with a top and bottom part, C and D to be ſcrewed on, air-tight, and taken off at pleaſure. In the center of the bottom part D, is a male ſcrew, which goes into a braſs nut in the mahogany pedeſtal E. The wires F and G are for the electric fire to paſs through, darting from one to the other. The wire G extends through the pedeſtal to H; and may be raiſed or lowered by means of a male ſcrew on it. The wire F may be taken out, and the hook I be ſcrewed into the place of it. K is a glaſs tube with a ſmall bore, open at both ends, cemented in the braſs tube L, which ſcrews into the top part C. The lower end of the tube K is immerſed in water, coloured with cocheneal, at the bottom of the tube A B. (I uſed at firſt coloured ſpirits of wine; but, in one of the experiments I made, it took fire.)

On the top of the tube K is cemented, for ornament, a braſs feril, with a head ſcrewed on it, which has a ſmall air hole thro' its ſide at *a*. The wire *b* is a ſmall round ſpring, that embraces the tube K ſo as to ſtay wherever it is placed. The weight M is to keep ſtrait whatever may be ſuſpended in the tube A B. on the hook I. Air muſt be blown thro' the tube K into the tube A B, 'till enough is intruded to raiſe, by its elaſtic force, a column of the coloured water, in the tube K up to *c*, or thereabouts; and then, the gage wire *b* being ſlipt down to the top of the column, the thermometer is ready for uſe.

EXP. VII.

I ſet the thermometer on an electric ſtand, with the chain N fixed to the prime conductor, and kept it well electriſed a conſiderable time; but this produced no ſenſible effect. Which ſhews, that the electric fire, when in a ſtate of reſt, has no more heat than the air and other matter wherein it reſides.

EXP. VIII.

When the wires F and G are in contact, a large charge of electricity ſent thro' them, even that of my caſe of five and thirty bottles, containing above thirty ſquare feet of coated glaſs, will produce no rarefaction of the air included in the tube A B. Which ſhews, that the wires are not heated by the fires paſſing thro' them.

EXP. IX.

When the wires are about two inches apart, the charge of a three pint bottle, darting from one to the

the other, rarefies the air very evidently. Which ſhews, I think, that the electric fire muſt produce heat in itſelf, as well as in the air, by its rapid motion.

The charge of one of my glaſs jars, which will contain about five gallons and a half, wine meaſure, darting from wire to wire, will, by the diſturbance it gives the air in the exploſion repelling it in all directions, raiſe the column in the tube K up to *d*, or thereabouts; and the charge of the above-mentioned caſe of bottles will raiſe it to the top of the tube. Upon the air's coaleſcing, the column, by its gravity, inſtantly ſubſides till it is in equilibrio with the rarefied air; it then gradually deſcends, as the air cools, and ſettles where it ſtood before. By carefully obſerving at what height above the gage-wire *b* the deſcending column firſt ſtops, the degree of rarefaction is diſcovered; which, in great exploſions, is very conſiderable.

EXP. X.

I hung in the thermometer, upon the hook I, ſucceſſively, a ſtrip of wet writing paper, a wet flaxen and woolen thread, a blade of green graſs, a filament of green wood, a fine ſilver thread, a very ſmall braſs wire, and a ſtrip of gilt paper; and found that the charge of the glaſs jar, paſſing thro' each of theſe, eſpecially the laſt, produced heat enough to rarify the air very perceptibly. The charge of the caſe of bottles ſent thro' the braſs wire conſumed great part of it into ſmoke. The thermometer appeared quite opaque with it.

EXP. XI.

I then ſuſpended, out of the thermometer, a piece of braſs wire, not quite ſo ſmall as the former, about twenty four inches long, with a pound weight at the lower end; and, by ſending the charge of the caſe of bottles thro' it, diſcovered a new method of wire-drawing. The wire was red hot, the whole length well anealed, and above an inch longer than before. A ſecond charge melted it; it parted near the middle, and meaſured, when the ends were put together, four inches longer than at firſt. This experiment I remember you propoſed to me, as worth trying, before you left Philadelphia; in order to find, whether the electricity, in paſſing thro' the wire, would ſo relax the coheſion of its conſtituent particles, as that the weight might produce a ſeparation; but neither of us had the leaſt ſuſpicion, that any heat would be produced.

EXP. XII.

That I might have no doubt of the wire's being *hot* as well as red, I repeated the experiment on another piece of the ſame wire, encompaſſed with a gooſe-quill filled with looſe grains of gun-powder; which took fire as readily, as if it had been touched with a read hot poker. Alſo tinder, tied to another piece of the wire, kindled by it. I tried a wire about twice as big, but could produce no ſuch effects with that.

Hence it appears, that the electric fire, tho' it has no ſenſible heat when in a ſtate of reſt, will, by its violent

violent motion, and the reſiſtance it meets with, produce heat in other bodies, when paſſing thro' them, provided they be ſmall enough. A large quantity will paſs thro' a large wire without producing any ſenſible heat; when the ſame quantity paſſing thro' a very ſmall one, being there confined to a narrower paſſage, the particles crowding cloſer together, and meeting with greater reſiſtance, will make it red hot, and even melt it.

Hence lightning does not melt metal by a cold fuſion, as we formerly ſuppoſed. But when it paſſes thro' the blade of a ſword, if the quantity be not very great, it may heat the point ſo as to melt it, while the broadeſt and thickeſt part may not be ſenſibly warmer than before.

And when trees or houſes are ſet on fire by the dreadful quantity, which a cloud, or the earth ſometimes diſcharges, muſt not the heat, by which the wood is firſt kindled, be generated by the lightning's violent motion thro' the reſiſting combuſtible matter?

If lightning, by its rapid motion, produces heat in itſelf as well as in other bodies, (and that it does, I think, is evident from ſome of the foregoing experiments made with the thermometer) then its ſometimes ſingeing the hair of animals killed by it may eaſily be accounted for. And the reaſon of its not always doing ſo may, perhaps, be this: the quantity, tho' ſufficient to kill a large animal, may, ſometimes, not be great enough, or not have met with reſiſtance enough, to become by its motion burning hot.

We find, that dwelling houſes, ſtruck with lightning, are ſeldom ſet on fire by it; but when it paſſes thro'

thro' barns with hay or ſtraw in them, or ſtore-houſes containing large quantities of hemp, or ſuch like matter, they ſeldom, if ever, eſcape a conflagration. Which may, perhaps, be owing to ſuch combuſtibles being apt to kindle with leſs degree of heat than is neceſſary to kindle wood.

We had four houſes in this city, and a veſſel at one of the wharfs, ſtruck, and damaged, by lightning laſt ſummer. One of the houſes was ſtruck twice in the ſame ſtorm. But I have the pleaſure to inform you, that your method of preventing ſuch terrible diſaſters, has, by a fact, which had like to have eſcaped our knowledge, given a very convincing proof of its great utility, and is now in higher repute with us than ever.

Hearing, a few days ago, that Mr. William Weſt, merchant in this city, ſuſpected, that the lightning, in one of the thunder-ſtorms laſt ſummer, had paſſed through the iron conductor, which he had provided for the ſecurity of his houſe, I waited on him, to enquire what ground he might have for ſuch ſuſpicion. Mr. Weſt informed me, that his family and neighbours were all ſtunned with a very terrible exploſion, and that the flaſh and crack were ſeen and heard at the ſame inſtant. Whence he concluded, that the lightning muſt have been very near; and, as no houſe in the neighbourhood had ſuffered by it, that it muſt have paſſed through his conductor. Mr. White, his clerk, told me, that he was ſitting at the time by a window, about two feet from the conductor, leaning againſt the brick wall, with which it was in contact; and that he felt a ſmart ſenſation, like an electric ſhock, in that part of his body, which touched the wall.

Mr.

Mr. Weſt further informed me, that a perſon of undoubted veracity aſſured him, that, being in the door of an oppoſite houſe on the other ſide of Water-Street (which you know is but narrow) he ſaw the lightning diffuſed over the pavement, which was then very wet with rain, to the diſtance of two or three yards from the foot of the conductor. And that another perſon of very good credit told him, that he, being a few doors off, on the other ſide of the ſtreet, ſaw the lightning above, darting in ſuch direction, that it appeared to him to be directly over that pointed rod.

Upon receiving this information, and being deſirous of further ſatisfaction, there being no traces of the lightning to be diſcovered in the conductor, as far as we could examine it below, I propoſed to Mr. Weſt our going to the top of the houſe to examine the pointed rod; aſſuring him, that, if the lightning had paſſed thro' it, the point muſt have been melted; and, to our great ſatisfaction, we found it ſo. This iron rod extended in height about nine feet and a half above a ſtack of chimnies, to which it was fixed; (but I ſuppoſe, three or four feet would have been ſufficient). It was ſomewhat more than half an inch diameter, in the thickeſt part, and tapering to the upper end. The conductor, from the lower end of it to the earth, conſiſted of ſquare iron nail rods, not much above a quarter of an inch thick, connected together by interlinking joints. It extended down the cedar roof to the eaves, and from thence down the wall of the houſe, four ſtory and a half, to the pavement in Water-Street; being faſtened to the wall, in ſeveral places, by ſmall iron hooks. The lower end was fixed

fixed to a ring in the top of an iron ſtake, that was driven about four or five feet into the ground. The above mentioned iron rod had a hole in the top of it, about two inches deep, wherein was inſerted a braſs wire, about two lines thick, and, when firſt put there, about ten inches long, terminating in a very acute point; but now its whole length was no more than ſeven inches and a half, and the top very blunt. Some of the metal appears to be miſſing; the ſlendereſt part of the wire being, as I ſuſpect, conſumed into ſmoke. But ſome of it, where the wire was a little thicker, being only melted by the lightning, ſunk down, while in a fluid ſtate, and formed a rough irregular cap, lower on one ſide than the other, round the upper end of what remained, and became intimately united therewith.

This was all the damage, that Mr. Weſt ſuſtained by a terrible ſtroke of lightning. A moſt convincing proof of the great utility of this method of preventing its dreadful effects. Surely it will now be thought as expedient to provide conductors for the lightning as for the rain.

Mr. Weſt was ſo good as to make me a preſent of the melted wire; which I keep as a great curioſity, and long for the pleaſure of ſhewing it to you. In the mean time, I beg your acceptance of the beſt repreſentation I can give of it; which you will find by the ſide of the thermometer, drawn in its full dimenſions as it now appears. The dotted lines above are intended to ſhew the form of the wire before the lightning melted it.

And now, Sir, I moſt heartily congratulate you on the pleaſure you muſt have in finding your great and well

well-grounded expectations ſo far fulfilled. May this method of ſecurity from the deſtructive violence of one of the moſt awful powers of nature meet with ſuch further ſucceſs, as to induce every good and grateful heart to bleſs God for the important diſcovery. May the benefit thereof be diffuſed over the whole globe. May it extend to the lateſt poſterity of mankind; and make the name of FRANKLIN, like that of NEWTON, *immortal.*

I am, Sir, with ſincere reſpect,

your moſt obedient, and

moſt humble ſervant,

Ebenezer Kinnerſley.

XXIII. *Obſervations in Electricity and on a Thunder-ſtorm: In a Letter from Mr.* Torbern Bergman, *to Mr.* Benjamin Wilſon, *F. R. S. Acad. Reg.* Upſal. *Soc.*

Ampliſſime atque Celeberrime Domine,

Read April 14, 1763. IN epiſtolis recentiſſimis, quibus me honorâſti, experimenta domini Delaval circa electricitatem cryſtalli Iſlandicæ commemoras. Pluries hæcce tentamina iteravi, ſed conſtanter eventu prorſus contrario. Scilicet in hunc finem varia hujus cryſtalli fruſta frigori 12 graduum expoſui,